ing on to or into the sun at all. And whatever be the real value of the arguments I used, I certainly had not before seen them anywhere clearly stated. The question as to meteors being consumed in producing heat by friction in the solar atmosphere, or of their striking the sun's proper surface in a solid form, is of very secondary importance, and would of course be determined chiefly by the size and nature of the meteoric bodies themselves.* The potential or heat result theoretically would be the same either way; though in the former case it might hardly so well explain how whilst heated flames are shot up 60,000 or 70,000 miles from the sun itself, that body is also probably far hotter than the upper regions of this very atmosphere, in which the meteors themselves would be giving out heat.

Prestwich, Manchester, Aug. 5 R. P. GREG

Kant's Transcendental Distinction between Affection and Function

AT page 80 of my "Revival of Philosophy at Cambridge," I ventured to describe a question set by Mr. Mahaffy, at Trinity College, Dublin, as "very oddly worded." I might have said *very improperly worded*, and have justified the sentence; but for the fact that it was the Cambridge examination-papers only that were the subject of my criticism in that work. My boldness in this censure on Mr. Mahaffy has occasioned remark. That gentleman is confessedly a capital metaphysician, perhaps of greater power than the learned professor whose work he translated and annotated. I therefore ask for space in NATURE to assign the reasons on which I asserted that his question was "very oddly worded." Here is the question: "Explain the statement that his [Kant's] doctrine of Space and Time is based on a *transcendental distinction*." I think I cannot be in error in taking this as a reference to the *Kritik der reinen Vernunft*, Transac. Æsth. § 8, *Allgemeine Anmerkungen*, &c., and in particular to the paragraph beginning "Die Leibnitz-Wolffsche Philosophie," &c., and that which immediately follows. In the former, and indeed in its immediate precursor, Kant is impugning the view that affection and function (Sense and Intellect) have only a logical difference, as if Sense were only differenced from Understanding by the inferiority of its representations, in precision and clearness. The latter paragraph (the third of those I have referred to) beginning "Wir unterschieden sonst wohl unter Erscheinungen das," &c., may be thus rendered:—

"We otherwise draw a proper distinction, in phenomena, between that which is an essential property of the intuition of it, and is generally valid for every one's sense, on the one hand; and on the other, that which belongs to it accidentally, inasmuch as it is not valid for the faculty of general sensibility, but only for a particular state or organisation of this or that sense."

This, Kant names an *empirical distinction*. To it he opposes what he calls a *transcendental distinction*, viz., that between phenomena and things in themselves, or what is involved therein, that between affection and function. As Kant points out, if we do not make the distinction between affection and function, we cannot explain the transcendental constituents of a phenomenon, and thereby we take it for a thing in itself, *i.e.*, a reality existing independently of our perception of it; and we so lose the distinction between the phenomenon and the thing in itself, of which latter we know nothing whatever.

Now, Mr. Mahaffy's question concerns a certain statement. Whose? Well, probably, it is his own, viz., that which occurs in a footnote to p. 57 of his translation of Fischer's Commentary on Kant's C. p. R. Mr. Mahaffy here says, "We must not confuse the *empirical* distinction between real object and merely subjective appearance with the *transcendental* distinction upon which his (Kant's) doctrine of space and time is based." This I believe to be the topic referred to in the question. It is manifestly open to two objections. Each of the terms "real object," and "merely subjective appearance" is equivocal. "Real object" may be the phenomenal object or the noumenal object. "Merely subjective appearance" may be what Kant calls "blosz Erscheinung" or the impression which the object makes on the particular sense of this or that subject. The actual terms used by Mr. Mahaffy more properly import the distinction between the thing in itself and its phenomenon; yet it is plain he means to follow Kant, and to speak of the distinction between phenomenal object, and particular subjective impression. It is the other matter of the extract with which the question deals; and it is this which I have no hesitation in pronouncing a very improper and misleading statement.

Kant's doctrine of Space and Time is *not* based on this distinction. On the contrary, the distinction is an outcome of that doctrine, and does not, cannot, emerge till that doctrine is established. The fruit of the doctrine cannot be its root; nor can that which is the basis of the distinction be itself based on that distinction. The truth is, that Kant's doctrine of Space and Time is one that concerns Sense only; it is Æsthetic; and as discovering the *a priori* sense-elements of experience, it is called Transcendental Æsthetic. Accordingly it touches but one pole of the distinction, and only so far helps it out! How can such a doctrine be properly said to be based on a distinction between the two poles? The very notion is preposterous, and derives, of course, no countenance whatever from the *Critic of pure Reason.*

The extremely curt and concise manner in which I have dealt with the actual subjects of examination in my book was essential to its brevity and corresponding lowness of price. The above remarks will show how insecure will be any inference of the poverty of my reasons from the paucity of my words.

Ilford, E., Aug. 1 C. M. INGLEBY

Spontaneous Generation

IF there is one thing more curious than another in the "Spontaneous Generation" theory, it is the way in which so-called matters of fact, as proved by careful experiment, are brought forward by the one side to be disproved by the other; one need only instance Pasteur's famous flask experiments, which were thought to be so overwhelming at the time, but which were afterwards refuted, I think by Frémy and others.

I notice with surprise the letters of Prof. Wanklyn and Dr. Lionel Beale in NATURE, with regard to the presence of germs in the air; there is an experiment of Pasteur's, given in his "Memoirs upon the organised Corpuscles which exist in the Atmosphere, 1862," and which I have never seen disproved, and if not disproved it must surely settle at least this part of the question. He passed a quantity of air, taking various precautions to eliminate error, which I need not here detail, by means of an aspirator through a plug of gun-cotton; he then dissolved the gun-cotton in ether, and on examining the sediment which subsided in the course of an hour or two, he found abundant evidence of the presence of organised corpuscles.

Bath CHARLES EKIN

Mirage

I HAVE just returned to England from H.M.S. *Porcupine*, having accompanied the dredging expedition as far as Lisbon. In reading the back numbers of NATURE, I notice in that for July 28 an account of an extraordinary mirage in the Firth of Forth on July 22. A reference to my journal shows me that on the same day we were dredging on the Portuguese coast, within sight of the Ferilhoe and Berlinga Islands, about forty miles north of Lisbon. The bearings of these islands and their exact distance, calculated by the aid of the known height of the lighthouse, gave us, of course, an exact position, which our "dead reckoning" also confirmed. Several solar observations, both for latitude and longitude, were taken by two of the officers during the day, both of whom always arrived at the same result, but this was so widely different from our position as previously determined by two other methods, that we were forced to the conclusion that there was a very false horizon. It was the only instance of the kind during the month I was at sea.

Clifton, Aug. 8 WM. LANT CARPENTER

The Sun's Corona

IF "my mathematical result was based upon data among which the principal point at issue was accepted as proved," it will be easy for you to state what that point is,* and to quote one passage at least in which Mr. Lockyer has associated it with his theory. In this way alone can you justify the assertion in your last editorial note.

So much you are bound in common justice to do. But further, it would be satisfactory if a distinct statement of Mr. Lockyer's

---

* Suppose the case of a large mass of meteoric iron falling through the solar atmosphere, the first result would of course be a development of heat and light; but if the original or cosmical velocity of the meteoric body was lost through the resistance of the atmosphere (as in similar cases terrestrially happens) *before* the entire mass was consumed, there might possibly result an actual loss of solar energy, caused by the subsequent and necessary melting of the residuum.

* A possible action at the moon's limb suggested by Faye, Gould and others.—ED.

© 1870 Nature Publishing Group

opinion respecting the corona could be made public. At present all that is generally known is, that he regards the corona as "an effect due to the passage of sunlight through our own atmosphere near the moon's place." Those are the words he used (see NATURE, vol. i., p. 14). I imagined that I had understood them. It seems I had not. Will he explain them, and perhaps indicate how the sunlight gets there? I only need to learn how one ray of sunlight can reach the atmosphere near the moon's place, during central totality in any considerable eclipse, and why the atmosphere actually *at* the moon's place (that is, all that cone of air which lies between the eye and the moon) is left free of this sunlight. This being satisfactorily explained, I should waive all other objections and accept the atmospheric glare theory without further question. RICHARD A. PROCTOR

[Mr. Proctor should have quoted the context, in which Mr. Lockyer carefully refers to Dr. Frankland's and his own conclusion (or theory) "against any such extensive atmosphere as the corona had been imagined to indicate." He then states that the "conviction" that the corona is probably an atmospheric effect, "is growing stronger and stronger," if the negative evidence of the new method of observation were alone taken into account; but this is not to elaborate a theory.—ED.]

The Horse-Chestnut

REFERRING to NATURE, No. 37, your correspondent, Eugene A. Connell, has fallen into a mare's nest in the matter of the horse-chestnut.

Country people are well aware of the impression of the horse's foot he has discovered, but the coincidence is quite accidental, and has nothing to do with the name.

"Horse" is a very common prefix, denoting largeness or coarseness, in the same way that the prefix "dog" indicates inferiority and contempt. Thus we have horse-chestnut, horse-bean, horse-radish, horse-mint, horse-parsley, horse-leech, dog-rose, dog-violet, dog-berry (the berry of *Solanum nigrum*), &c.

These prefixes are common to nearly all languages; we have ἱππό-κρημνος, a horse-laugh, "fièvre de cheval," a violent fever, and a host of like terms.

Bath, July 27 CHARLES EKIN

The "English Cyclopædia"

IF the Editor of the "English Cyclopædia," in his letter contained in your issue of July 7, had restricted himself to defending his own handiwork, and had abstained from denying the correctness of my statements, I should not have ventured to ask for space in your columns to reply to him.

In opposition to my statement that I looked in vain for "*Arvicolæ, Crociduræ, Crossopi, Hypudæi, Sorices*, shrews, and voles," the Editor asserts that "all the species mentioned in the Close Time Report are described or noticed in the Cyclopædia." This may be and probably is quite true. I merely asserted that they were not to be found under their respective names. I have stated that I found *Hypudæus* and the voles under the heading *Muridæ*. In return for my information he now tells me that if I wished to become acquainted with *Crocidura* and *Crossopus* I ought to have turned to the article *Sorecidæ*. But how is an unlearned reader like myself to know where to turn? The Editor only confirms the accuracy of my statement as to the great want of cross references. If the Index and the Supplement had contained such references as *Hypudæus* [Muridæ, E.C.], *Crossopus* [Sorecidæ, E.C.], &c., I should probably never have given public utterance to my troubles. In reply to my assertion that I looked in vain for an article on *Rhizocrinus*, I am told, much to my astonishment, that the proper place to find it is under *London Clay*. In my ignorance I had sought for it under its own name, *Apiocrinites*, and *Encrinites*. According to this mysterious system of arrangement, if I had complained that there was no article on *Sparrows*, I should probably have been told that I ought to have looked for a notice of them under the heading *London*.

In my letter I gave a list of twenty-three important subjects on which there were no articles. In defence of these real or apparent omissions the Editor, after making the strange assertion that two of these, *Acclimatisation* and *Deep Sea Dredging*, belong rather to the "Arts and Sciences" than to the "Natural History" division, goes on to say that most of the subjects stated by me to have been omitted "do occur." He must be well aware that I never asserted that they "do not occur." I simply said that there were no special articles on them. He might have had the candour to notice that I unearthed from their hidden recesses the subjects to which he expressly refers in his letter, viz., *Eophyton, Eozoon, Hyalonema*, and *Protoplasm*.

As I must not trespass further on your space, I will conclude by observing that I fully concur with the Editor in the opinion that "what a Cyclopædia ought or ought not to contain is an open question;" but when an Editor has the moral courage to assert, in illustration of the mode in which he discharged his functions, that "*Meloe* was inserted and *Sphegidæ* rejected, because there was not room for both," and gives no less than twelve columns to the former instead of dividing the space between the two; and when he tells us that *London Clay* is the proper place to seek for information regarding *Rhizocrinus*, the readers of NATURE may draw their own inferences as to what a Cyclopædia, under his superintendence, is likely to be.

I must add that I have not the slightest idea who the Editor of the Supplement is, and that until his letter appeared, I did not believe in his existence.

South Devon, July 8 NEMO

Entomological Inquiries, etc.

I WAS much interested, two nights ago, at finding on the wall of my drawing-room a flattish, dark-grey winged insect, six or seven tenths of an inch in length, which, on being placed in the hand, exhibited two small but brilliant sparks of light towards the extremity of the tail. In the imperfect light in which it was examined, the wings seemed to have elytra and the body to be somewhat like a small caterpillar, with a tapering tail. In size and general aspect it resembled the Italian fire-fly, with which I made acquaintance last summer on the Lake of Como, without, however, a sufficient examination to justify more than the most superficial comparison. My knowledge of entomology is so defective, that I feel unable to form an opinion whether it might be that insect or the male of the common glow-worm (which, however, is not common in my neighbourhood). If so meagre a description may enable any of your readers to give me satisfactory information as to this point, I shall feel much obliged to them.

There is adequate evidence that some kind of fire-fly has been seen during hot seasons in England. Kirby and Spence give a reference, which I have no opportunity of verifying, to Phil. Trans. 1684, as to their appearance in Hertfordshire, and their having been considered the genuine *Lampyris italica*. The following unpublished account may be interesting as having come to me from a perfectly reliable source: "In 1822—the year is pretty certain—during the month of June or July, the weather being very hot, on at least two evenings a number of fire-flies were seen at a village near the Thames, between Reading and Henley; they were flying about the fields and the lawn before a gentleman's house, and some of them came into the house; three or four or more might be seen at a time, like little flying lamps. The insect was brown [reference is then made to a sketch from memory thirty-two years afterwards, from which it must have greatly resembled my specimen], and seems to have had opaque elytra and network wings; the light was in the tail, like that of a glowworm, as bright, but probably not as large. A very intelligent gentleman who was upon the spot, an acquaintance of Dr. Wollaston's, who had been in America and the West Indies, was greatly astonished; he caught some of them, and considered them identical with the West Indian firefly. He said he had heard of their being in England, but never seen them."

A lady, whose experience must be referred to a later date than the foregoing account, has informed me that she once observed them for a single day in Wiltshire.

The newspapers of 1868 or 1869—I am not certain which—spoke of them as abundant in some places; particularly, I think, at Caversham in Kent, where they were even considered "nuisances!" if I recollect right. Some of the readers of NATURE may perhaps be able to furnish information as to this alleged fact.

There is something very remarkable in the occasional appearance of these beautiful insects in our climate. They can hardly be thought to reach us by direct migration. Can it be supposed—as it has been ingeniously suggested to me—that their ova are frequently being imported from warmer countries, but are only fully developed in the temperature of our hottest summers?

© 1870 Nature Publishing Group